

An Instrumental Variables Approach for Estimating the Incidence of Endogenous Policies*

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Abstract

An important issue in public policy analysis is the likely endogeneity of the policies under study. If policy changes constitute responses on the part of legislators to changes in a variable of interest, then standard analyses that treat policy changes as natural experiments may yield biased estimates of the impact of the policy. We examine the extent to which such political endogeneity biases conventional fixed effects estimates of behavioral parameters by identifying the elasticities of demand for cigarettes and beer using the timing of state legislative elections as an instrument for changes in state excise taxes. In both cases, we find sizable differences between these estimated demand elasticities and fixed effect estimates, suggesting that standard fixed effects estimators may be unable to identify the causal effects of state-level policy changes.

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1. Introduction

A critical, but rarely addressed, issue affecting the empirical analysis of public policies is the likely endogeneity of the policies under study. If, as seems plausible, many policy changes constitute responses on the part of legislators to changes in a variable of interest (henceforth, labeled the “outcome” variable), then standard analyses that treat policy changes as exogenous may yield biased estimates of the impact of the policy. For example, if recent decisions by state governments to raise cigarette excise taxes were based in part on changes in smoking behavior, then studies that treat changes in state excise taxes as natural experiments might lead to biased inferences regarding the impact of taxes on cigarette demand.

Although recognized for some time, policy endogeneity has only recently begun to attract significant attention in the literature. As discussed in a recent paper by Besley and Case (2000), a majority of studies that analyze the impact of public policies treat variation in state-level policy variables as exogenous.¹ Most of these studies employ either fixed effects or difference-in-difference estimators, which rely on within-state variation in the policy and outcome variables to identify the effect of the policy change. However, as discussed by Besley and Case (2000), neither approach addresses the fundamental problem associated with endogenously-determined policies, which is the endogenous response of policymakers to within-state changes in either the outcome variable itself, or an unobserved factor, like voter sentiment, which independently influences the outcome variable.²

¹ Examples include: Anderson and Meyer (1997), who analyze the effect of unemployment insurance rates on employment and earnings; Blank et al. (1996), who examine the impact of state abortion laws on abortion rates; and Card (1992), who studies the effect of state minimum wage laws on employment. Most relevant for the present paper are the numerous studies on tobacco and alcohol consumption which utilize variation in state excise taxes for identification. Examples include: Becker, Grossman, and Murphy (1994), Chaloupka and Wechsler (1997), and Gruber (2000), who estimate price elasticities for cigarettes; and Cooke and Tauchen (1982), Saffer and Grossman (1987), Ruhm (1996) and Dee (1999), who estimate the influence of alcoholic beverage taxes on a variety of alcohol-related health outcomes.

² Although fixed effects specifications remove any endogeneity stemming from time-invariant differences in state characteristics, including differences in the fixed proclivities of state policymakers, problems remain if policy changes are prompted by changes over time in either the outcome variable itself or the attitudes of policymakers toward the outcome variable. The use of state-specific trends as a control variable may mitigate these effects, but need not eliminate them if policymakers respond to shocks which move the outcome variable away from its trend. For example, legislators may be content to remain passive in the face of a slowly evolving trend in a variable like youth smoking, but may be driven to take action following a sudden “spike” in the variable.

One method for dealing with policy endogeneity is to incorporate an explicit model of the policymaking process into the analysis. While such an approach holds the promise of providing a deeper understanding of the joint determination of government policies and their effects, specifying such a model is quite difficult and may require an understanding of political institutions that is outside the experience of most researchers.³

Besley and Case (2000) take a first step in this direction. They use the gender composition of state legislatures to instrument for the actuarial cost of state workers' compensation benefits when estimating the effect of benefit generosity on employment and earnings. While their analysis demonstrates a significant impact of female political participation on workers' compensation benefits, it remains unclear whether or their instrument can be treated as exogenous. For example, it may be the case that states with more rapidly growing economies, and therefore rising employment and wages, may also be more predisposed to elect female legislators. Although this does not appear to be a problem in the workers' compensation example analyzed in their paper, as a general rule, using political *outcomes* as instruments for policy choices would seem to be a precarious identification strategy.

In this paper, we propose an alternative method for dealing with policy endogeneity based on the use of an instrumental variable similar to the one first used by Levitt (1997) in his study of the effect of police on crime rates. In that paper, policy endogeneity was a significant concern because cities with high or rising crime rates are likely to respond by hiring more police officers than cities with low or falling crime rates, thereby frustrating attempts to determine the causal influence of a larger police force on crime. Levitt's solution was based on a pair of observations that we believe carry over in a general way to state policymaking. First, policymakers behave differently in election years than in non-election years; and second, the timing of elections is fixed, and therefore should not be correlated with the outcome of interest, except through its affect on the policy variable.

³ A novel approach is contained in a recent paper on welfare reform by Grogger (2000), who controls for the possible endogeneity of state welfare reforms by specifying a fixed effect for each state-year pair, relying on the variation in the age structure of families within states in a given year to identify the effects of the reforms.

There are several reasons why legislators might behave differently in election years than in other years. First, there is much anecdotal evidence that incumbent office-holders are reluctant to pass controversial bills during election years. Such sentiments are often expressed in media reports and are frequently cited as one reason for “gridlock” in election years. Conversely, in instances where the proposed policy change enjoys widespread support, one would expect legislators to be *more* likely to enact the change in an election year than in a so-called “off” year. In either case, it seems intuitive that legislators will seek to capture any political advantage that can be gained by strategically timing the passage of legislation.

A second reason why legislators might behave differently in election years than in non-election years is the additional competition for legislators’ time created by the need to campaign and raise funds. *Ceteris paribus*, we would expect that these added time demands would result in a slowing of the legislative process.

Underscoring the existence of a relationship between the timing of elections and the timing of public policy changes is evidence from a number of studies which document the existence of electoral cycles at both the state and local levels (Mikesell (1978); Berry and Berry (1992); Poterba, (1994); Besley and Case (1995)); Levitt (1997)). At the state level, Mikesell (1978) found that states were more likely to increase taxes in the years immediately following an election than in election years. Berry and Berry (1992) find that proximity to a gubernatorial election is the strongest and most consistent factor influencing the probability that a new tax will be adopted. Similarly, Poterba (1994) finds that both tax increases and spending reductions are smaller in gubernatorial election years than in other years, while Besley and Case (1995) show that the presence of a gubernatorial term limit induces an electoral cycle in state fiscal policy. At the local level, Levitt (1997) finds that city police forces grow more in mayoral and gubernatorial election years than in other years.

Because his focus was on city crime rates, Levitt (1997) used mayoral and gubernatorial election cycles to instrument for the number of police hired at the local level. Here, we focus on state-level

policies, and use the timing of state legislative elections⁴ to instrument for two important policy changes initiated by state governments in recent years: changes in state cigarette excise taxes and changes in state excise taxes on beer. We then use our IV estimator to estimate the effect of each of these policy changes on a relevant outcome variable (consumption of cigarettes and beer, respectively), and compare our IV estimates to estimates derived from standard fixed effects models that treat changes in state excise taxes as exogenous.

In both cases, we find that the timing of state legislative elections is a significant determinant of changes in state excise taxes, and that the estimated impact of taxes on consumption is substantially larger when the changes in taxes (or prices) are instrumented. In the case of cigarette consumption, the estimated price elasticity of demand more than doubles (from -0.46 to -1.07), while in the case of beer consumption, the price elasticity rises by a factor of six (from -0.18 to -1.08). While we do not claim that the models estimated in this paper provide the best possible estimates of the parameters of interest, our results do suggest that standard econometric models currently used to evaluate policy changes may be unable to identify the causal effects of those policies. We offer our instrumental variables approach as an easily implemented alternative that may be useful for addressing concerns about policy endogeneity in a variety of settings.

The remainder of the paper is organized as follows. In Section 2, we focus on the demand for cigarettes, and demonstrate that failing to account for policy endogeneity can lead to a large bias in the estimated price elasticity of demand. Given this finding, we extend our analysis in Section 3 by examining the demand for beer, another product that has been the object of considerable scrutiny by policymakers in recent years. As in the case of cigarettes, we find a sizeable difference in the estimated impact of a change in taxes when the endogeneity of state tax policy is taken into account. In section 4, we offer concluding remarks.

⁴ Note that in all but a handful of cases, state legislative elections, which typically occur every two or four years, occur in the same years as state gubernatorial elections.

2. Cigarettes

The large and burgeoning literature on tobacco consumption and control has had as its main focus the price elasticity of demand for cigarettes (Chaloupka and Warner (1999)). This emphasis reflects both the perceived importance of price as a policy tool and the greater ease with which the influence of price can be evaluated relative to other control policies (e.g. advertising restrictions, counter advertising, and clean indoor air laws). This literature is also one that has struggled with fundamental identification issues, and one that has embraced many of the empirical methodologies discussed earlier in the paper.

Many early studies on the demand for cigarettes treated cigarette prices as exogenous (Baltagi and Levin (1986); Wasserman et al. (1991)). Others have attempted to identify the impact of price on demand using variation in state excise taxes, which have also been treated as exogenous (Becker, Grossman, Murphy (1994); Chaloupka and Wechsler (1997); Gruber (2000)). The majority of these more recent studies have improved upon earlier work by including state and year fixed effects in their models, and, in some cases, state-specific trends (Evans and Huang (1998)). As discussed in the introduction, however, these methodological advances will not always be sufficient to protect the analysis from the biases associated with policy endogeneity. For example, if states that experience unusually rapid growth in cigarette demand are more likely to increase excise taxes, then standard analyses that treat changes in state cigarette excise taxes as exogenous will understate the (negative) impact of price on consumption. Conversely, if rising cigarette demand makes states less likely to increase cigarette taxes, then conventional fixed effects models will likely overstate the reduction in cigarette consumption resulting from a tax increase. This problem has largely been overlooked in the tobacco literature; a recent exception being Ohsfeldt et al. (1998), who instrument for state cigarette taxes using several state-level political and economic variables.⁵

⁵ In particular, they use per capita spending, per capita tobacco production, a measure of political ideology within the state, and an index of competition among political parties as instruments for state cigarette taxes. Although one may question the exogeneity of several of these variables, it is interesting to note that the authors find a larger impact of taxes on the probability of smoking using their instrumental variables approach than when cigarette taxes are treated as exogenous.

In this section, we document the existence of an electoral cycle in state cigarette excise taxes and use the election cycle as an instrument to examine whether conventional estimates of the price elasticity of cigarette demand are biased. We begin by using state-level panel data to estimate a standard demand equation that treats cigarette prices as exogenous. Next, we instrument for price using state excise taxes, a widely-used methodology that relies on the assumption that state cigarette taxes are exogenously determined. We then compare these estimates to those obtained using the election cycle as an instrument for price, finding evidence that policy endogeneity does exist in this context, and that a failure to account for it can result in a substantial underestimate of the likely impact of taxes on consumption.

2.1. Data

The cigarette data used are a panel of the 50 U.S. states, with yearly observations running from 1955 to 1997. Information on per capita cigarette consumption, cigarette prices and excise taxes by state is taken from the publication *Tax Burden on Tobacco* published by the Tobacco Institute.⁶ Demographic data on state per capita income over time are taken from Bureau of Labor Statistics (BLS) sources.

Summary statistics of this panel are presented in Table 1. Slightly fewer than 12 packs of cigarettes are smoked per person per month over this time period. A pack of cigarettes costs about a dollar and a half on average (measured in 1997 dollars), and excise taxes (both state and federal) accounted for about forty percent of the price on average.⁷

Figure 1 shows the time series (in logs) of per capita cigarette consumption in the sample. Consumption increased until the early 1960s, then remained approximately constant for two decades.

⁶ Data on cigarette consumption is not available for all states for the early years of the panel. The following is a list of the states that do not have consumption data starting in 1955 and when the consumption data for that state begins: Alaska 1959, California 1960, Colorado 1965, Hawaii 1960, Maryland 1959, Missouri 1956, North Carolina 1970, Oregon 1967 and Virginia 1961. If we complete our analysis using only the years of the data which are complete (1970 and onward), we obtain similar results to those presented below. Also, because it does not have the same election system as the 50 states, we do not include data for the District of Columbia. Therefore, we have 2086 complete state/year observations in the panel.

⁷ We measure the excise tax of a state in June of the year. This month is chosen because we want yearly changes in a state's excise tax to be measured at the same time as the changes in cigarette prices. Our results are similar if we use other months in the year.

Since the early 1980s, there has been a precipitous decline in cigarette consumption. Per capita cigarette consumption in 1997 was only sixty-five percent of consumption in 1981.

The time series (also in logs) of cigarette prices and excise taxes in the sample is presented in Figure 2. There is a high positive correlation in the movement of cigarette prices and excise taxes. Cigarette prices fell in the 1970s as the real value of excises taxes eroded with inflation. Since the early 1980s, real cigarette prices steadily rose until they peaked in the mid-1990s. Cigarette prices in 1997 were 170% higher than the 1981 price. State and federal taxes increased substantially over this same time period.

2.2. Using Tax Changes to Measure the Price Elasticity of Cigarette Consumption

Figures 1 and 2 show that cigarette consumption decreased substantially starting in the 1980s as cigarette prices were increasing to their highest levels, suggesting that there might be an important effect of cigarette prices on cigarette demand. We attempt to measure the causal effect of prices on cigarette consumption by estimating a regression of changes in state cigarette consumption on changes in state cigarette prices. The regression equation is:

$$\Delta \ln \text{Packs per person}_{i,t} = \alpha + \beta \Delta \ln \text{Price per pack}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (1)$$

where $\text{Packs per person}_{i,t}$ is the number of packs of cigarettes per person per month consumed in state i in year t ; $\text{Price per pack}_{i,t}$ is the average price of a pack of cigarettes in state i in year t . Both variables are log-differenced in the regression specification. $X_{i,t}$ is a log-differenced measure of state per capita income. φ_t is a set of year dummies, and $\varepsilon_{i,t}$ is the error term.⁸

⁸ This empirical specification is widely used in the tobacco literature (see, for example, Evans and Huang (1998); Evans and Ringel (1999); Farrelly (2000); or Gruber (2000)). One potential problem is that we do not control for either cross-state cigarette smuggling or state smoking regulations. The issue of smuggling from low to high-tax states has received much attention in the literature. However, a recent paper by Farrelly (2000), which contains the richest set of controls to date for smuggling activity, finds that estimated price elasticities are not appreciably affected by omitting controls for cross-border sales. A similar conclusion is reached by Evans and Ringel (1999),

The coefficient of interest is β , which measures the effect of changes in state cigarette prices on changes in state cigarette consumption. The OLS estimate of equation (1) is presented in column (1) of Table 2. The coefficient on changes in state cigarette prices suggests that a 10 percent increase in cigarette prices is associated with about a 4 percent decrease in cigarette consumption. In column (2), we add state effects to the regression specification which, because the data are differenced, control for linear state time trends in cigarette consumption. The correlation between changes in cigarette prices and cigarette consumption is almost identical to column (1).

To give β a causal interpretation as a demand elasticity, we need to instrument changes in state cigarette prices with a factor that affects cigarette prices but does not affect cigarette demand in any other way. A standard candidate in the cigarette demand literature for this instrument is a measure of changes in state cigarette excise taxes.

In column (3) of Table 2, we present the regression estimates of equation (1) using 2SLS with $\Delta \ln \text{Excise tax}_{i,t}$ as an instrument. The coefficient on $\Delta \ln \text{Price per pack}_{i,t}$ is negative and statistically different from zero. The implied demand elasticity of cigarette consumption is -0.51 .⁹ In column (4), we again add state effects to the regression specification. The estimated demand elasticity decreases slightly in absolute value to -0.46 compared to our previous estimate in column (3). These estimates are consistent with the range of -0.30 to -0.50 cited by Chaloupka and Warner (1999) as encompassing the majority of recent price elasticity estimates for cigarettes.¹⁰

who also demonstrate that excluding controls for state smoking regulations does not affect the estimated impact of taxes on smoking.

⁹ Changes in state excise taxes are highly correlated with changes in cigarette prices in the first-stage estimation. As shown at the bottom of Table 2, the F statistic of the instrument in the first-stage estimation is very high. As with many other studies (for example Harris (1987) and Keeler *et al.* (1996)), we find that on average increases in state excises taxes lead to more than 100% pass through of the tax to cigarette prices.

¹⁰ It should be noted that this range applies primarily to estimates of the total price elasticity of demand. The total elasticity is a measure of the responsiveness of the total number of cigarettes purchased (by all consumers) to a change in price. Studies based on microdata often decompose the total elasticity into a participation elasticity, which measures the sensitivity of the probability of smoking to price, and a conditional demand elasticity, which measures the price responsiveness of current smokers. The total elasticity incorporates both of these effects. Although it would be desirable to examine participation and conditional demand elasticities as well, to do so using our election cycle instrument would require a relatively long panel of individual-level data that could be matched to

An important assumption underlying these IV estimates is that state legislatures are not influenced by changes in cigarette demand within the state when determining changes in cigarette excise taxes. If states are responding to cigarette demand changes when setting taxes, then cigarette demand elasticities estimated using tax changes as instruments can be biased.

There are several reasons why states might take demand changes into account when determining excise tax levels. First, public health concerns about the dangers of smoking might cause states to increase excise taxes during periods of increasing cigarette demand. Under such a scenario, using excise taxes as an instrument would bias the estimated elasticity upward (towards zero). Warner (1981) and Chaloupka and Warner (1999) argue that there are several periods over the last fifty years when U.S. states and other countries have responded to public health concerns when setting excise taxes.

Also, state governments might take into account the revenue or political implications of changing cigarette demand when determining taxes. For example, if cigarette demand is growing, then states might be enticed to increase cigarette taxes to take advantage of the greater revenue that will be raised. On the other hand, higher cigarette demand might mean that more voters would be upset by a cigarette tax increase, lessening the chance that state legislatures will vote for such increases.

These scenarios suggest that using state excise tax changes to identify the effect of price on cigarette demand might not measure the true causal effect of price on demand. In what follows, we use the state election cycle as an instrument for the state cigarette price to avoid political endogeneity problems when measuring the demand elasticity.

2.3. Election Cycles in State Cigarette Excise Tax Changes

state-level data on cigarette excise taxes. To the best of our knowledge, the only data set that meets all of these requirements is the Monitoring the Future Survey, which tracks high school students over time. Unfortunately, the public use version of this data set does not contain state identifiers.

Legislative election cycles vary across states for a couple of reasons. First, some states have statewide legislative elections every two years while other states only have elections every four years.¹¹ Also, most states schedule their elections on even calendar years, but there is a significant minority of states that hold elections in odd years.

There are several reasons why one might expect to observe a link between election timing and the timing of cigarette excise tax changes. First, taxes are often a critical political issue in elections; legislators might be reluctant to vote on and pass any tax increases during an election year, or they might not want tax increases to take effect during an election year. The tax increase might alienate voters in general or smokers in particular. On the other hand, if smokers are an unimportant voting minority, then legislators might want to increase cigarette taxes during election years if additional revenue is needed by the state to avoid raising taxes that hurt more politically important constituencies.¹² Second, because legislators are spending time campaigning during an election year, they might devote less time to passing laws. Because legislators generally only increase cigarette excise taxes in nominal terms, a paralysis of the legislative process during election years suggests that excise tax changes in the subsequent year should be smaller in real terms than in other years.

Empirically, changes in state excise taxes, and therefore changes in state cigarette prices, do tend to be affected by election cycles. We measure whether changes in state cigarette prices are different following election years than other years. The regression equation is:

$$\Delta \ln \text{Price per pack}_{i,t} = \alpha + \beta \text{Election previous year}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (2)$$

¹¹ Like the U.S. Congress, a typical state has an upper and lower house. Usually, the entire lower house is up for election each election cycle and part of the upper house is up for election. Nebraska is the only state with a unicameral legislature.

¹² Scharff (2000) presents evidence that smokers have become a less important voting block over time due to their declining numbers, lower propensity to vote, and reduced likelihood of being marginal voters.

where $Election\ previous\ year_{i,t}$ is an indicator that the state held a legislative election the previous year and the other variables are defined as before. Here β measures whether cigarette prices change differently after election years in a state compared to other years.

The results of this regression are presented in column (1) of Table 3. The coefficient on the indicator of a state having a legislative election is negative and statistically different from zero, suggesting that cigarette prices increase less after election years compared to other years. This result might suggest that legislators are hesitant to vote for a cigarette tax increase during an election year that takes effect the next year; alternatively, it might suggest that legislators want tax increases to take effect *during* election years to please anti-smoking advocates. The magnitude of the coefficient indicates that changes in cigarette prices are slightly less than 1 percent lower after election years than after non-election years. In column (2), state effects are also included in the regression specification; the effect of elections on changes in cigarette prices is almost identical to the estimate in column (1).

This relationship between the timing of elections and changes in cigarette prices is caused by the fact that states change their excise tax less after election years than in other years. The propensity of states not to increase cigarette excise taxes after election years also emerges when the data is analyzed on a year-by-year basis. Figure 3 displays a plot of the difference in the changes in excise taxes for states with and without elections the previous year. While there is substantial year-to-year variability in the average change in excise taxes, states with elections the previous year exhibit lower tax changes in 33 of the 43 years of our sample.

Another way of examining the robustness of the relationship between cigarette excise tax changes and elections is to analyze the data on a state-by-state basis. A full list of states, along with information on mean changes in cigarette excise taxes after election and non-election years is provided in Appendix Table 1. In 41 of the 50 states, the mean change in excise taxes in a state after election years is lower than after non-election years.

Given that cigarette price changes are lower after election years, then if price changes affect cigarette consumption, a reduced-form relationship between elections and cigarette consumption should emerge. The reduced-form specification is:

$$\Delta \ln \text{Packs per person}_{i,t} = \alpha + \beta \text{Election previous year}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (3)$$

where the variables are defined as above. The estimates of equation (3) are shown in column (1) of Table 4. The coefficient on the election indicator is positive and statistically different from zero, indicating that cigarette consumption grows in states the year after an election. The estimates imply that cigarette consumption grows about 0.73 percent more after election years than after non-election years. When state effects are added to the regression specification, the estimates suggest that consumption grows slightly less than 1 percent more after an election year in a state compared to other years.

2.4. Using Election Cycles to Estimate the Effect of Price on Cigarette Consumption

The preceding section demonstrates a negative correlation between elections and changes in cigarette prices, as well as a positive correlation between changes in cigarette consumption and elections. Together, those results suggest a direct relationship between cigarette prices and consumption that is examined in this section using election timing as an instrument for changes in cigarettes prices.

The impact of cigarette prices on consumption is estimated using 2SLS, treating cigarette prices as endogenous and the other right-hand-side variables as exogenous. The particular form of the equation to be estimated is equation (1) defined above. The results from estimating this equation using the election instrument are presented in column (1) of Table 5. The effect of cigarette prices on consumption is again negative and statistically different from zero. The elasticity implied by the coefficient is -1.03 and is bigger in absolute value than the estimate using tax changes as an instrument. In column (2), the regression specification also includes state effects. The elasticity increases in absolute value to -1.07 .

Both estimates are about twice as large in absolute value as the estimated elasticity using tax changes as the instrument instead of election cycles.

We next expand the instrument set by allowing the effect of elections to vary across census regions of the U.S. Columns (3) and (4) present 2SLS estimates of the effect of cigarette prices on consumption using a set of nine region/election interactions as instruments. In both specifications, the estimated demand elasticity is very similar to estimates obtained using only the election cycle as an instrument.

The bottom of Table 3 reports the F statistic and the partial R^2 of the election cycle instruments in the first-stage estimations. Except for column (3), the instruments have a statistically significant effect on changes in cigarette prices; however, they explain much less of the variation in cigarette prices than changes in state excise taxes. Bound, Jaeger and Baker (1995) and others have shown that there are potential problems with instrumental variables estimates when there is a low correlation between the instrument and the endogenous explanatory variable.

First, with a “weak instrument”, the finite-sample bias of 2SLS might be severe, with the IV estimates biased toward the OLS results. However, in our estimates, instrumenting with the election cycle indicator moves the price elasticity estimate farther away from the OLS estimate than using the change in the excise tax as an instrument. Therefore, if finite-sample bias is a problem with our estimates, this would suggest that the true price elasticity is even farther from the conventionally-estimated elasticity than our estimates would indicate, implying that policy endogeneity is an even greater problem than it would appear based on our estimates.

In many circumstances, LIML performs better than 2SLS when there is finite-sample bias because of “weak instruments” (see Angrist, Imbens and Krueger (1999)). In columns (5) and (6) of Table 5, we present the LIML estimates of the elasticity of demand for cigarettes using the region/election interactions as instruments. In column (5), the price coefficient appears exaggeratedly negative and the standard error blows up; this is probably not surprising given the sensitivity of LIML to the particular choice of specification. When state effects are added to the LIML specification, the instruments perform

much better (as shown in column (6)) and the price elasticity estimate is very similar to the 2SLS estimates.

Second, even a weak correlation between the instrument and the error term of the second-stage regression can lead to large inconsistencies in IV estimates if the instrument is weak. If the state election cycle is correlated with changes in cigarette consumption for reasons other than changes in cigarette prices, then demand elasticities estimated using the election indicator as an instrument might be flawed.

It is difficult to describe a relationship between the timing of state elections and state cigarette demand that is not connected to changes in cigarette prices. Probably the best story is one that involves a relationship between state election timing and the adoption of other state anti-smoking policies. For example, if states are more likely to enact restrictive public indoor smoking laws or produce anti-smoking ad campaigns before elections, and these interventions affect cigarette demand, then our IV strategy might attribute the effect of these policies on demand to price changes.

To determine if this is a problem with our empirical strategy, we re-estimate our cigarette demand regressions using only the first twenty years of our sample period (1955-1974). During this period, there were few if any attempts by states to control smoking demand through non-price means such as clean indoor air laws or ad campaigns. Using this smaller sample, we obtain qualitatively the same cigarette demand estimates as with the full sample. Therefore, it does not appear that our results are being driven by an election cycle in other state cigarette policies.

Another story is one that involves state elections leading to an electoral cycle in state fiscal policy, as described by Besley and Case (1995). If there are political business cycles at the state level, and if changes in cigarette demand are related to state economic conditions, then changes in cigarette demand might be correlated with election timing. If this were the case, and we were unable to control for state economic conditions in our regression specification, then our estimated demand elasticities might be biased. Although it seems unlikely that political business cycles at the state level would have a

quantitatively important impact on cigarette consumption, we nonetheless include state per capita income as a control variable in all of our models.¹³

Third, several studies have shown that conventional standard errors can be inaccurate when there are “weak instruments” (see, for example, Staiger and Stock (1997)). Hahn and Hausman (forthcoming) have developed a new specification test to determine if the conventional IV asymptotics are reliable in a given situation. Their test involves comparing the 2SLS coefficient of the endogenous regressor to the reciprocal of the 2SLS regression where the endogenous regressor and the left-hand-side variable are switched. Under the null hypothesis that conventional first order asymptotics are accurate, the two estimates are similar. Using the election indicator interacted with census regions as our set of instruments, we cannot reject the hypothesis that the forward and reverse 2SLS regressions produce similar estimates, suggesting that our standard errors are reliable.¹⁴

As an additional specification check, we performed an overidentification test using the region/election interactions as instruments. To implement the test, we took the residuals from the second-stage regressions of the 2SLS estimates shown in columns (3) and (4) in Table 5 and regressed them on the instruments and all of the exogenous variables in the model. The test statistic of the validity of the overidentifying restrictions is computed as $N \times R^2$, where N is the number of observations and R^2 is the unadjusted R^2 from the regression of the residuals on the exogenous variables and the instruments. The test statistic is distributed χ^2 with degrees of freedom equal to the number of overidentifying restrictions. In both cases, the overidentifying restrictions could not be rejected (p-value = .26 when state effects were not included (column (3)), and p-value = .82 when state effects were included (column (4))).

2.5. Discussion

¹³ We have examined how our results change if we include lags and leads of changes in per capita income in the regression specification. These additional state economic controls do not greatly change our demand elasticity estimates.

¹⁴ This test requires that the system be overidentified.

The doubling of the price elasticity estimate that we find when using the election cycle as an instrument points to a potential bias in conventional estimates that treat variation in state cigarette excise taxes as exogenous. We interpret the large change in this coefficient as evidence that changes in state-level cigarette taxes represent either direct responses to changes in the level of cigarette demand in the state, or indirect responses to an unobserved variable, such as anti-smoking sentiment, that jointly influences both tax rates and demand. In either case, models that rely on the exogeneity of excise taxes for identification are likely to lead to biased inferences regarding the impact of taxes on consumption.

The magnitude of these biases can be illustrated by comparing the effects of a hypothetical \$0.45 increase in cigarette prices on cigarette consumption. This experiment corresponds closely to the coordinated increase in cigarette prices that accompanied the signing of the Master Settlement Agreement between participating tobacco companies and the Attorneys General of several states in late 1998.¹⁵ The average price of cigarettes prevailing on November 1, 1998 was \$2.18 per pack. Thus, a \$0.45 per pack price increase translates into an average percentage increase of 20.6 percent. If one multiplies this figure by -0.46 (our “conventionally estimated” price elasticity¹⁶ from Table 2, column 4), one arrives at a predicted reduction in cigarette consumption of 9.48 percent. In contrast, if one uses our IV estimate of -1.07 based on the election cycle (Table 5, column 2), the predicted decline in cigarette consumption more than doubles to 22.04 percent.

Another way of illustrating the differing implications of the two elasticity estimates is to calculate the reduction in smoking-related mortality associated with the same \$0.45 per pack increase in cigarette prices used in the previous example. To perform a rough, “back-of-the-envelope” calculation of these mortality effects, we use 2SLS regression results from Moore and Hughes (2001), who estimate the short-

¹⁵ It should be noted that the predictions generated by our model correspond to the effects of a *ceteris paribus* change in price on consumption. Other factors may lead actual results to differ. An additional complication, not accounted for in our calculations, is that not all tobacco companies had implemented the price increase as of April 2000 (for details on the Master Settlement Agreement, see Cutler et al. (2000)).

¹⁶ Observe that this estimate lies within the -0.30 to -0.50 consensus range of estimates cited by Chaloupka and Warner (1999).

run¹⁷ reduction in smoking-related mortality associated with a given decline in per capita cigarette consumption. Their results imply that a one-pack reduction in annual per capital cigarette consumption is associated with an annual decline in smoking-related mortality of 0.753 lives per 100,000 persons. Combining this estimate with our conventionally estimated price elasticity estimate, we calculate a total reduction in smoking-related mortality of 14.54 per 100,000 persons, or 39,340 lives in 1998. When the larger elasticity estimate is used, this number grows to 33.81 per 100,000 persons, or 91,477 lives in 1998; a difference of 52,137 lives saved.

3. Beer

In this section, we investigate whether the bias found in the conventionally estimated cigarette price elasticity is peculiar to that example, or whether biases associated with policy endogeneity are likely to be a more general problem. To do so, we examine alternative estimates of the price elasticity of the demand for beer, another product that has attracted considerable attention from policymakers in recent decades. The literature on alcoholic beverage consumption shares with the tobacco literature a central focus on the role of taxation as a control policy. Although numerous other control policies have been analyzed (e.g. minimum legal drinking ages, advertising restrictions, etc.), the most robust finding to emerge is the inverse relationship between beverage prices (or taxes) and consumption (Cook and Moore (1999)). In the case of beer, most estimates of the own-price elasticity of demand are clustered around a range of -0.20 to -0.40 (Duffy (1990); Johnson et al. (1992); Duffy (1995); Nelson and Moran (1995); Clements et al. (1997); Salisu and Balasubramanyam (1997); Nelson (1999); Cooke and Moore (1999)). Although most of these estimates are derived from time series studies using aggregate data, it is worth

¹⁷ It is important to bear in mind that the Moore and Hughes estimate only applies to the immediate (one-year) reduction in smoking-related mortality stemming from a given change in per capita cigarette consumption. It is unclear whether the long-run effects would be larger or smaller. Although one is tempted to assume that mortality reductions would be larger if a longer time horizon were considered, it seems likely that a portion of the contemporaneous mortality effect is due to persons whose smoking-related illnesses are simply delayed for several years. For such persons, the contemporaneous reduction in aggregate mortality would be offset in later years.

noting that the OLS price elasticity estimates that we obtain using state panel data (-0.18 and -0.59) lie quite close to this range.

We perform a similar comparison to the one presented in the previous section for cigarettes. We begin by estimating a simple OLS model that treats state beer taxes as exogenous. Using the results from this model, we calculate the price elasticity estimates referenced above. Next, we instrument for beer taxes using the election cycle as our instrument, and compare the associated 2SLS estimates of the beer price elasticity to those derived from ordinary least squares. As in the case of cigarettes, we find substantially larger price elasticities when the election cycle is used as an instrument, leading us to conclude that conventionally estimated price elasticities for beer are likely biased by a failure to take into account the endogenous nature of state tax policy.

3.1. Data

The beer data used are a panel of 49 states, with yearly observations running from 1970 to 1997.¹⁸ Information on per capita beer consumption is taken from publications by the National Institute on Alcohol Abuse and Alcoholism. Excise taxes by state are from publications of the Distilled Spirits Council of the United States, and data on state per capita income over time are taken from BLS sources. Unfortunately, information on beer prices by state are not available for this time period.¹⁹

Summary statistics for this panel are presented in Table 6. On average, slightly fewer than 30 gallons of beer are consumed per capita over this time period, and excise taxes per gallon of beer averaged a little more than \$1 in 1997 dollars.

Figure 4 shows the time series (in logs) of per capita beer consumption in the sample. Consumption increased until the early 1980s, then has decreased steadily for the next two decades. The

¹⁸ Data from Hawaii are not used in our analysis because, for several years during our sample period, Hawaii imposed an *ad valorem* tax on beer. Without information on beer prices in Hawaii, we do not know the value of the tax.

¹⁹ Beer prices are collected quarterly across states by the American Chamber of Commerce Research Association (ACCRA); however, their price series begins in 1982. Also, the ACCRA price data are only for one brand of beer and there are significant gaps in the data for various states and years.

time series (also in logs) of beer taxes in the sample is presented in Figure 5. Except for a large increase in the federal excise tax in 1991, beer taxes have steadily fallen over the course of the sample period.

3.2. OLS Estimates of the Price Elasticity of Beer Consumption

Unlike our cigarette application, Figures 4 and 5 show that there does not appear to be a strong relationship in the movements of beer taxes and consumption over time. However, we attempt to measure a causal effect of taxes on beer consumption in a more formal way by estimating a regression of state beer taxes on state per capita beer consumption. The regression equation is:

$$\ln Gallons\ per\ person_{i,t} = \alpha + \beta \ln Tax\ per\ gallon_{i,t} + X_{i,t}\delta + \gamma_i + \varphi_t + \varepsilon_{i,t} \quad (4)$$

where $Gallons\ per\ person_{i,t}$ is the number of gallons of beer consumed per person in state i in year t . $Tax\ per\ gallon_{i,t}$ is the state excise tax on beer in state i and year t . γ_i is a full set of state effects, and the other variables are defined the same as before.²⁰

The coefficient of interest is β , which measures the elasticity of beer consumption with respect to changes in the beer tax. The OLS estimate of equation (4) is presented in column (1) of Table 7. The coefficient on state beer taxes is negative and statistically different from zero, indicating that a 10 percent increase in beer taxes is associated with about a 0.80 percent decrease in beer demand. In column (2), we add linear state trends to the regression specification; the coefficient on beer taxes falls by about two-thirds, suggesting that a 10 percent increase in excise taxes decreases beer consumption by about 0.24 percent.

²⁰ We estimate this regression in levels instead of differences because we want to minimize potential biases associated with measurement error. It is well-known that differencing exacerbates any measurement error in the explanatory variables. This was not an issue in the cigarette application because there the “benchmark” model was estimated using 2SLS, which results in consistent estimates even in the presence of measurement error. Because we do not have price data for beer, our benchmark model in the present case is one in which beer consumption is regressed directly on beer taxes. When we compare the estimated elasticity from this regression to one based on our election cycle instrument, we would like to minimize the chances that any difference between the two is due to measurement error in the OLS estimate.

For purposes of comparison with the existing literature, we converted these tax elasticities into price elasticities under the assumption that beer taxes are passed through to prices on a one-for-one basis.²¹ Given this assumption, we computed a price elasticity for each tax elasticity estimate using averages for beer consumption (29.23 gallons per person per year) and state beer taxes (\$1.05 per gallon in 1997 dollars) over our sample period. To complete the conversions, we need information on the average beer price in effect over the sample period, which we estimate as \$7.74 per gallon (in 1997 dollars).²² Calculated at these sample averages, the OLS price elasticity estimates are -0.18 and -0.59 for the specifications with and without state-specific trends, respectively. These estimates are quite close to other price elasticity estimates for beer found in the literature (Cook and Moore (1999)).

An important assumption underlying these OLS estimates is that changes in state beer excise taxes are not influenced by changes in beer consumption. If states are responding to beer demand when setting taxes, then demand elasticities identified using variation in state beer taxes over time can be biased. As with the cigarette example, state legislators might be motivated by either public health concerns or revenue needs when setting beer taxes; therefore, we use the state election cycle as an instrument for state beer taxes to avoid these potential political endogeneity problems.

3.3. Election Cycles in State Beer Excise Tax Changes

²¹ In markets characterized by imperfect competition, taxes may be more than fully shifted to consumers (Besley, 1989; Katz and Rosen, 1985). In the case of retail sales taxes, Poterba (1996) found approximately one-for-one shifting, while Besley and Rosen (1999) find partial pass-through for some products and greater than 100 percent pass through for others. Young and Bielinska-Kwapisz (2000) argue that results based on sales taxes may not generalize to excise taxes, and provide evidence that alcohol taxes may be significantly over-shifted. For beer, their preferred estimate implies that beer prices increase by \$1.71 for each \$1.00 increase in beer taxes. Using 1.71 instead of 1.00 in our price elasticity calculations leads to elasticities of -0.10 and -0.34 for the OLS models with and without state trends, and -0.63 and -0.93 for the 2SLS models with and without state trends.

²² To calculate this average, we took the average nominal beer price for 1997 and used the beer CPI to construct nominal beer prices for each year in the sample. We then used the overall CPI to convert the nominal price for each year into 1997 dollars. Averaging these numbers resulted in an average beer price of \$7.74 per gallon (in 1997 dollars) for our sample period.

We first measure whether there is a relationship between the election cycle and beer tax changes, examining whether states change beer taxes differently after election years compared to other years. The regression equation is:

$$\Delta \ln Tax \text{ per gallon}_{i,t} = \alpha + \beta Election \text{ previous year}_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (5)$$

where the variables are defined as above. Here β measures whether beer excise taxes change differently in years following state legislative elections than in other years.

The results of this regression are presented in column (1) of Table 8. The coefficient on the state election indicator is positive and statistically significant from zero, indicating that beer taxes increase more after election years than after non-election years. Legislators might be reluctant to have tax increases *take effect* during election years, or state elected officials might want to appease anti-alcohol groups by passing beer tax increases during election years that take effect the following year. The magnitude of the coefficient suggests that changes in beer taxes are about 2.4 percent higher after election years than in other years. Adding state effects to the regression specification in column (2) does not change this conclusion.

The propensity of states to increase beer taxes after election years also emerges when the data is analyzed on a year-by-year basis. Figure 6 displays a plot of the difference in the changes in excise taxes for states with and without elections the previous year. States with elections in the previous year increase their beer tax more, on average, than other states during 20 of the 27 years in our sample.

We can also examine the relationship between beer excise tax changes and elections by analyzing the data on a state-by-state basis. A full list of states, along with information on mean changes in beer taxes after election and non-election years is provided in Appendix Table 2. In 42 of the 49 states, the mean change in beer taxes is greater after election years compared to non-election years.

Given that beer excise taxes are higher after state election years, then if taxes affect beer consumption, a reduced-form relationship between elections and beer consumption should emerge. The reduced-form specification is:

$$\Delta \ln Gallons\ per\ person_{i,t} = \alpha + \beta Election\ year\ previous\ year_{i,t} + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (6)$$

where the variables are defined as above. The estimates of equation (6) are shown in column (1) of Table 9. The coefficient on the election indicator is negative, suggesting that beer consumption falls about 0.59 percent more after election years than in other years. When state effects are added to the regression specification in column (2), the negative relationship weakens slightly.

3.3. Using Election Cycles to Estimate the Effect of Price on Beer Consumption

The preceding section demonstrates a positive relationship between elections and changes in beer taxes, as well as a negative correlation between changes in beer consumption and elections. Together, those results suggest a direct relationship between beer excise taxes and consumption that is examined in this section using election timing as an instrument for changes in beer taxes.

The impact of beer taxes on consumption is estimated using 2SLS. The regression specification is:

$$\Delta \ln Gallons\ per\ person_{i,t} = \alpha + \beta \Delta \ln Tax\ per\ gallon + \Delta \ln X_{i,t} \delta + \varphi_t + \varepsilon_{i,t} \quad (7)$$

where the variables are defined as above.²³ The results from estimating equation (7) using the election cycle instrument are presented in column (1) of Table 10. The effect of beer taxes on consumption is again negative; the tax elasticity implied by the coefficient is -0.25, which is about three times bigger in absolute value than the corresponding OLS elasticity estimate. Adding state effects to the regression

²³ Because the data are differenced, the state fixed effects serve as a control for linear state trends in beer consumption, thereby preserving comparability between the 2SLS specification and the OLS specification employed earlier.

specification does not greatly change the elasticity estimate; however, in both regression specifications the coefficients are not statistically different from zero.

We again expand the instrument set by allowing the effect of elections to vary across the nine census regions. Columns (3) and (4) present the 2SLS estimates using this larger instrument set. Again, the estimated tax elasticity is much larger than the corresponding OLS estimate, and the precision of the estimates is much greater; both tax elasticities are now statistically different from zero. Using the same procedure as was used for the OLS estimates, we converted the estimated tax elasticities into price elasticities, obtaining estimates of -1.08 and -1.59 for the specifications with and without state effects. These elasticity estimates are approximately six and three times larger than their respective OLS counterparts.

One concern with our estimates is that part of the difference between our OLS and 2SLS results might be because of measurement error in the OLS estimate. With classical measurement error, OLS coefficients are biased toward zero; instrumenting solves this problem. At this point, we do not know how much of the difference between our estimates is due to political endogeneity problems and how much is due to measurement error. But in either case, our estimated beer elasticities are much larger in absolute value than the usual estimates.

As in the cigarette application, there is the possibility that other policy interventions may be correlated with the timing of elections. In the case of beer, the most important policy changes occurring during our sample period (other than the tax changes that we analyze) are changes in state minimum legal drinking ages.²⁴ To examine whether changes in the minimum legal drinking age (MLDA) are confounding our results, we re-estimated our 2SLS models using only the period (1989 – 1997) during which the MLDA was uniform across states.²⁵ The tax elasticity estimates that we obtain are essentially

²⁴ See Dee (1999) for an analysis of the effect of the minimum legal drinking age on youth traffic fatalities.

²⁵ All but seven states had adopted a MLDA of 21 by 1989. The exceptions (Colorado, Idaho, Louisiana, Minnesota, Montana, Ohio, and Vermont) were dropped from the analysis.

the same as those obtained for the entire sample, suggesting that changes in the MLDA are not biasing our results.

Again, we need to worry about the potential problems of “weak instruments”. Because our 2SLS estimates are far away from the OLS estimates, if there is significant finite-sample bias, then this would suggest that the true elasticity is even farther away from the OLS result than our findings would indicate. We also present LIML estimates of the tax elasticity in columns (5) and (6) of Table 10. In both specifications, the estimated elasticity is close to our 2SLS estimates and much larger in absolute value than the OLS estimates. As with our cigarette application, we again perform the Hahn and Hausman (forthcoming) test to determine whether the conventional standard errors are accurate. Using the election cycle indicator interacted with census region, we cannot reject that the forward and reverse 2SLS estimates are similar, suggesting that there is no problem using the first order asymptotics.

Finally, we performed overidentification tests for the case where we used the region/election interactions as instruments. In the specifications with and without state effects, the overidentifying restrictions could not be rejected (p -value = .59 with state effects and p -value = .89 without state effects).

4. Conclusions

We have shown that in the case of two so-called “sin” taxes, utilizing an instrumental variables approach that uses only exogenous variation in state-level tax changes leads to substantially larger estimates of the price elasticities of demand than those derived from other commonly used methodologies (e.g. fixed effects models). These findings are consistent with a policy environment in which state legislators increase excise taxes on beer and cigarettes whenever the demand for these goods is high relative to their long-run trends. While we cannot draw any firm conclusions about the exact motives for this type of behavior, two possibilities immediately spring to mind. First, policymakers may be concerned with the public health dimensions of alcohol and tobacco use, and may increase taxes in an effort to curtail consumption whenever consumption appears to be growing at an unusually high rate. Alternatively, it may be that policymakers are not concerned about the health consequences of alcohol

and tobacco use, but instead look to products with growing demand as attractive targets for revenue-enhancing tax increases.

Regardless of which motive is at work, our findings indicate that it may be problematic to treat state-level policy changes as having been exogenously determined for purposes of public policy analysis. Instead, policy changes are best viewed as purposive responses on the part of policymakers to changes in the outcome variable being studied, or perhaps to some third factor that simultaneously influences both the policy and outcome variables. While different policies are likely to suffer from different degrees of political endogeneity, and therefore must be assessed on a case-by-case basis, our results suggest caution when analyzing policy changes that have attracted widespread attention, or that seem especially politically sensitive.

On a more positive note, this paper provides a simple approach for dealing with policy endogeneity that may prove useful in a variety of settings where policy changes cannot plausibly be viewed as “natural experiments.” The virtues of this approach are its simplicity, the readily available nature of the data, and its potential generalizability. A priori, it seems quite reasonable to believe that other policy variables may be subject to electoral cycles; perhaps the best known example being the election cycle in police hiring documented by Levitt (1997). A potential drawback is that relatively long panels of data may be required in order to generate sufficient variation in the instruments and endogenous explanatory variables.

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Table 1: Summary Statistics of Cigarette Consumption Data

	Mean	Minimum	Maximum
	(1)	(2)	(3)
Cigarette Consumption Per Capita (packs per person per month)	11.70 [2.95]	4.27	29.14
Price of Cigarettes (per pack: 1997 dollars)	1.50 [0.26]	0.87	2.65
Excise Tax on Cigarettes (per pack: 1997 dollars)	0.60 [0.17]	0.17	1.11
State Income (per capita: 1997 dollars)	17703 [4966]	6123	35863

Notes: The sample is yearly data on all U.S. states between 1955 and 1997. Data on cigarette consumption, prices and taxes are from the Tobacco Institute. Data on state per capita income are from the Bureau of Labor Statistics. Standard deviations are in brackets.

**Table 2: Estimates of the Elasticity of Cigarette Consumption
With Respect to Price Using Changes in Cigarette Excise Taxes as an Instrument**

	OLS		2SLS	
	(1)	(2)	(3)	(4)
$\Delta \ln$ Price of Cigarettes	-.3966 (.0235)	-.3852 (.0234)	-.5076 (.0783)	-.4586 (.0795)
$\Delta \ln$ State Income per Capita	.1360 (.0376)	.1160 (.0378)	.1335 (.0379)	.1148 (.0379)
Year Effects	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes
Instrument			$\Delta \ln$ Excise Tax on Cigarettes	$\Delta \ln$ Excise Tax on Cigarettes
F statistic of instrument in first stage			199.25	185.97
p-value of instrument in first stage			<0.001	<0.001
Partial R^2 of instrument in first stage			.0386	.0368

Notes: Dependent variable is $\Delta \ln$ Cigarette Consumption per capita. The number of observations is 2036.

Table 3: The Election Cycle as a Predictor of Changes in Cigarette Prices

	(1)	(2)
Indicator that State Held Legislative Election Previous Year	-.0073 (.0030)	-.0087 (.0031)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is $\Delta \ln$ Price of Cigarettes. Number of observations is 2036.

Table 4: The Election Cycle as a Predictor of Changes in Cigarette Consumption

	(1)	(2)
Indicator that State Held Legislative Election Previous Year	.0073 (.0034)	.0092 (.0035)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is $\Delta \ln$ Cigarette Consumption per capita. The number of observations is 2036.

Table 5: Estimates of the Elasticity of Cigarette Consumption With Respect to Price Using the Election Cycle as an Instrument

	2SLS				LIML	
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln$ Price of Cigarettes	-1.034 (.5083)	-1.066 (.4473)	-1.328 (.4642)	-.9505 (.2616)	-3.118 (1.665)	-1.124 (.3222)
$\Delta \ln$ State Income per Capita	.1219 (.0455)	.1053 (.0458)	.1156 (.0512)	.1071 (.0432)	.0758 (.1111)	.1044 (.0467)
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes	No	Yes
Instrument	Election Indicator	Election Indicator	Region \times Election Interactions	Region \times Election Interactions	Region \times Election Interactions	Region \times Election Interactions
F statistic of instruments in first stage	5.78	7.66	1.01	2.26		
p-value of instruments in first stage	0.016	0.006	0.433	0.016		
Partial R^2 of instruments in first stage	.0013	.0016	.0020	.0044		

Notes: Dependent variable is $\Delta \ln$ Cigarette Consumption per capita. The number of observations is 2036.

Table 6: Summary Statistics of Beer Consumption Data

	Mean	Minimum	Maximum
	(1)	(2)	(3)
Beer Consumption Per Capita (gallons per person per year)	29.23 [5.68]	13.33	50.66
Excise Tax on Beer (per gallon: 1997 dollars)	1.05 [0.52]	0.38	4.38
State Income (per capita: 1997 dollars)	19925 [3768]	10809	35863

Notes: The sample is yearly data on all U.S. states between 1970 and 1997. Data on beer consumption are from the National Institute on Alcohol Abuse and Alcoholism. Data on state beer taxes are from the Distilled Spirits Council of the United States. Data on state per capita income are from the Bureau of Labor Statistics. Standard deviations are in brackets.

**Table 7: OLS Estimates of the Elasticity
of Beer Consumption With Respect to Excise Taxes**

	(1)	(2)
ln State Beer Tax	-.0796 (.0081)	-.0243 (.0074)
ln State Income per Capita	.2497 (.0376)	.3227 (.0309)
Year Effects	Yes	Yes
State Effects	Yes	Yes
Linear State Trends	No	Yes

Notes: Dependent variable is ln Beer Consumption per capita. The number of observations is 1323.

Table 8: The Election Cycle as a Predictor of Changes in Beer Taxes

	(1)	(2)
Indicator that State Held Legislative Election Previous Year	.0243 (.0094)	.0247 (.0097)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is $\Delta \ln$ Beer Tax. Number of observations is 1323.

Table 9: The Election Cycle as a Predictor of Changes in Beer Consumption

	(1)	(2)
Indicator that State Held Legislative Election Previous Year	-.0059 (.0032)	-.0053 (.0033)
Year Effects	Yes	Yes
State Effects	No	Yes

Notes: Dependent variable is $\Delta \ln$ Beer Consumption per capita. The number of observations is 1323.

Table 10: Estimates of the Elasticity of Beer Consumption With Respect to Excise Taxes Using the Election Cycle as an Instrument

	2SLS				LIML	
	(1)	(2)	(3)	(4)	(5)	(6)
$\Delta \ln$ Beer Excise Tax	-.2506 (.1557)	-.2248 (.1527)	-.2153 (.0642)	-.1463 (.0719)	-.2398 (.0703)	-.2797 (.1221)
$\Delta \ln$ State Income per Capita	.1766 (.0535)	.1722 (.0514)	.1813 (.0479)	.1813 (.0448)	.1780 (.0496)	.1659 (.0539)
Year Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Effects	No	Yes	No	Yes	No	Yes
Instrument	Election Indicator	Election Indicator	Region \times Election Interactions	Region \times Election Interactions	Region \times Election Interactions	Region \times Election Interactions
F statistic of instruments in first stage	6.70	6.49	4.14	2.75		
p-value of instruments in first stage	0.010	0.011	<0.001	0.004		
Partial R^2 of instruments in first stage	.0050	.0048	.0272	.0161		

Notes: Dependent variable is $\Delta \ln$ Beer Consumption per capita. The number of observations is 1323.

Appendix Table 1: Average Changes in Real State Cigarette Excise Taxes

	After Election Year	After Non-Election Year		After Election Year	After Non-Election Year
Alabama	-0.69	0.23	Montana	-1.15	0.77
Alaska	-1.10	1.26	Nebraska	-0.69	1.45
Arizona	2.35	-0.17	Nevada	-1.18	1.99
Arkansas	-1.52	1.32	New Hampshire	-0.62	0.95
California	0.52	0.61	New Jersey	-1.27	2.32
Colorado	-1.46	1.75	New Mexico	-0.17	0.03
Connecticut	-1.78	3.32	New York	-0.29	2.10
Delaware	-0.70	0.99	North Carolina	-0.24	0.01
Florida	0.48	-0.30	North Dakota	-0.77	1.15
Georgia	-0.38	0.10	Ohio	-0.71	1.28
Hawaii	0.22	1.95	Oklahoma	-0.37	0.04
Idaho	-0.10	0.58	Oregon	0.26	1.00
Illinois	-1.57	2.81	Pennsylvania	-1.72	2.05
Indiana	-0.84	0.72	Rhode Island	1.89	0.16
Iowa	-0.93	1.78	South Carolina	-0.66	0.14
Kansas	-0.13	0.42	South Dakota	-1.30	2.02
Kentucky	-0.35	-0.36	Tennessee	-1.22	0.41
Louisiana	-1.28	-0.47	Texas	-0.91	1.72
Maine	-1.19	1.81	Utah	-0.89	1.01
Maryland	-1.02	0.99	Vermont	-1.25	2.21
Massachusetts	1.91	0.28	Virginia	-0.32	-0.43
Michigan	3.54	-0.83	Washington	-0.82	3.60
Minnesota	-1.52	2.67	West Virginia	0.76	-1.09
Mississippi	-0.61	0.05	Wisconsin	-0.87	2.12
Missouri	-0.56	0.85	Wyoming	-0.80	0.80

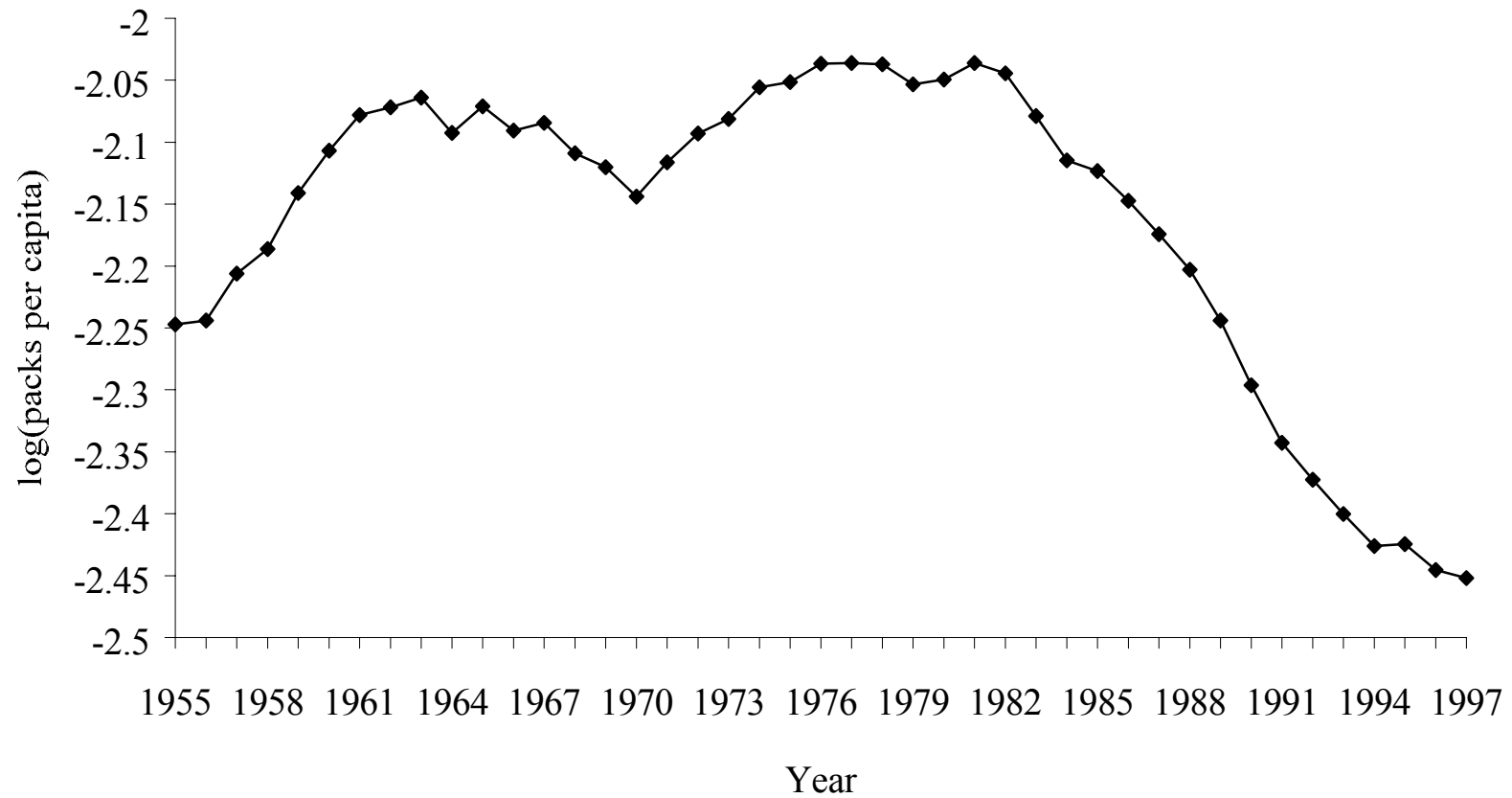
Notes: Changes in cigarette excise taxes are denominated in 1997 cents.

Appendix Table 2: Average Changes in Real State Beer Excise Taxes

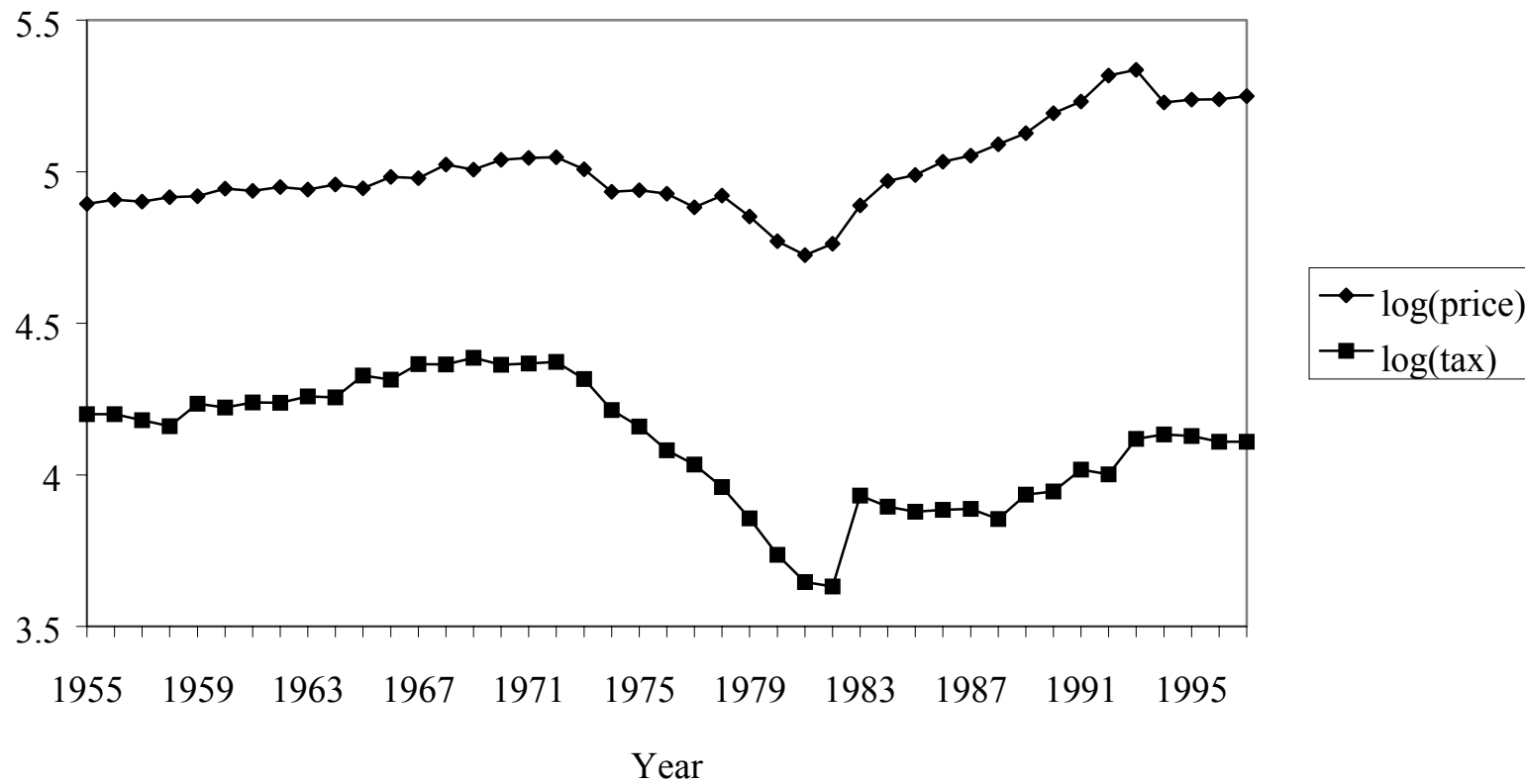
	After Election Year	After Non-Election Year		After Election Year	After Non-Election Year
Alabama	-6.26	-6.16	Montana	-0.73	-1.19
Alaska	-1.85	-3.26	Nebraska	0.16	-0.94
Arizona	-1.07	-0.17	Nevada	-0.39	-0.80
Arkansas	-2.70	-2.93	New Hampshire	0.09	-1.61
California	0.80	-0.60	New Jersey	0.35	-0.45
Colorado	-0.81	-0.43	New Mexico	1.31	-0.80
Connecticut	0.05	-1.13	New York	-0.03	-0.15
Delaware	-0.81	0.02	North Carolina	-5.94	-6.46
Florida	-1.76	-4.59	North Dakota	-1.79	-1.94
Georgia	-5.36	-5.81	Ohio	-1.63	-1.95
Hawaii			Oklahoma	-3.77	-3.10
Idaho	-1.67	-1.82	Oregon	0.13	-0.82
Illinois	-0.78	-0.85	Pennsylvania	-0.89	-0.97
Indiana	-0.43	-1.20	Rhode Island	-0.45	-0.82
Iowa	-1.06	-1.21	South Carolina	-8.57	-9.30
Kansas	-0.39	-2.02	South Dakota	-2.92	-2.96
Kentucky	-0.96	-0.91	Tennessee	-1.04	-1.39
Louisiana	-3.63	-3.78	Texas	-1.20	-1.63
Maine	-2.79	-3.02	Utah	0.43	-1.84
Maryland	-0.89	0.14	Vermont	-2.64	-3.07
Massachusetts	-0.74	-1.22	Virginia	-1.69	-2.72
Michigan	-2.27	-2.46	Washington	1.12	-0.74
Minnesota	-0.51	-1.59	West Virginia	-1.98	-2.15
Mississippi	-4.47	-5.16	Wisconsin	-0.36	-0.39
Missouri	0.22	-0.73	Wyoming	-0.22	-0.24

Notes: Changes in beer excise taxes are measured per gallon of beer and are denominated in 1997 cents.

**Figure 1: Cigarette Consumption Over Time:
1955-1997**



**Figure 2: Cigarette Prices and Taxes
Over Time: 1955-1997**



**Figure 3: Difference in Changes in Cigarette Taxes
(States with Elections Versus States with no Election)**

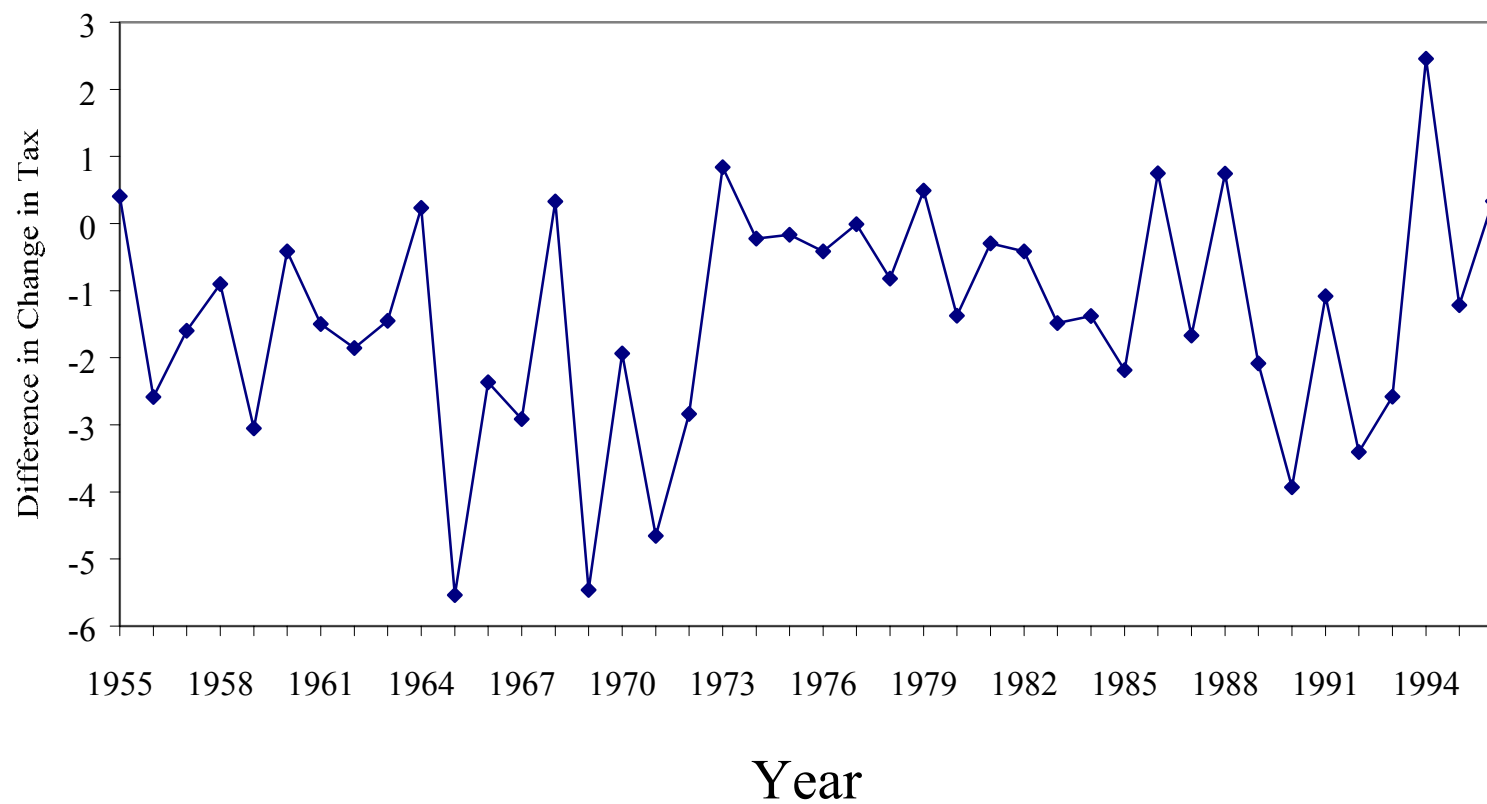


Figure 4: Beer Consumption Over Time: 1970-1997

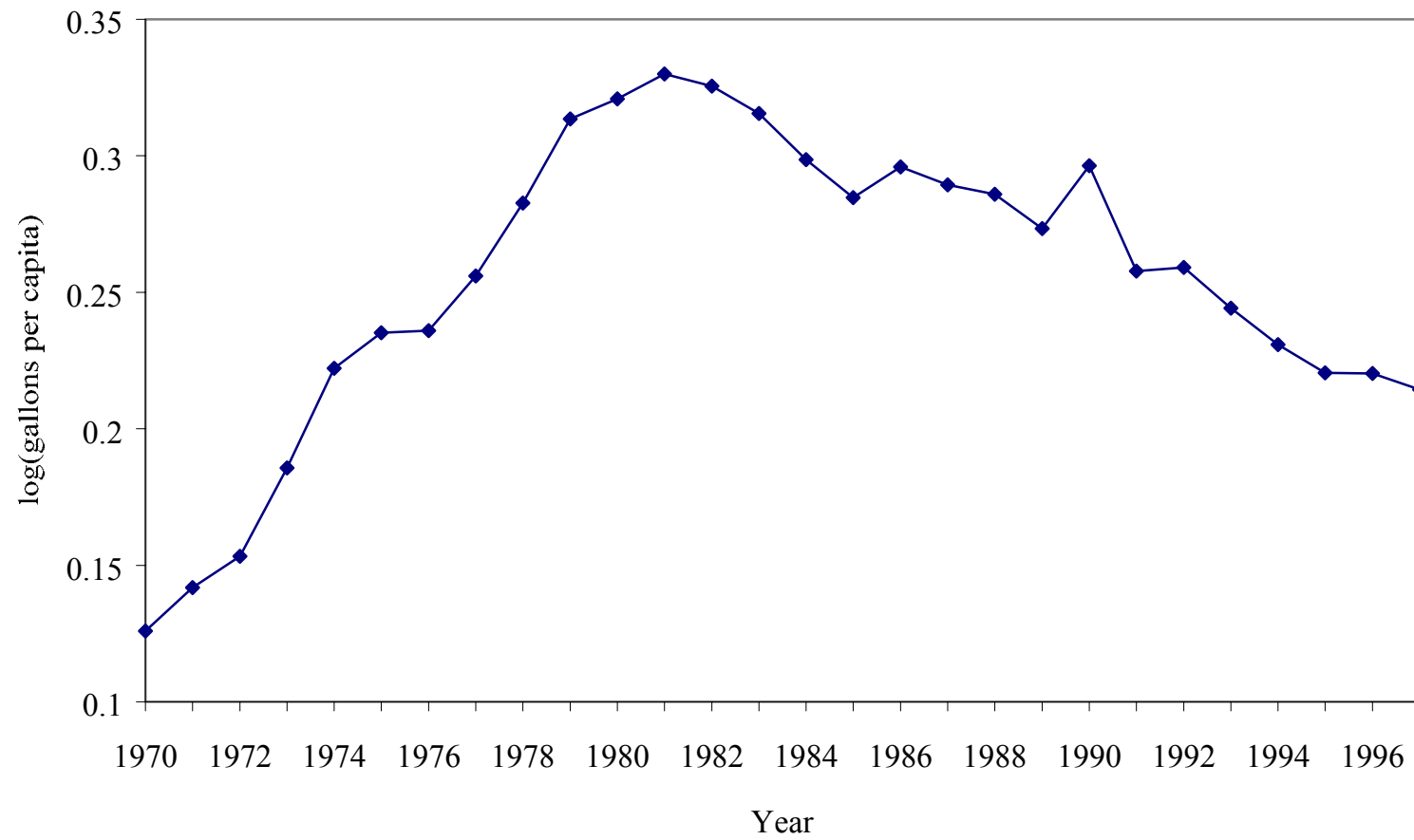
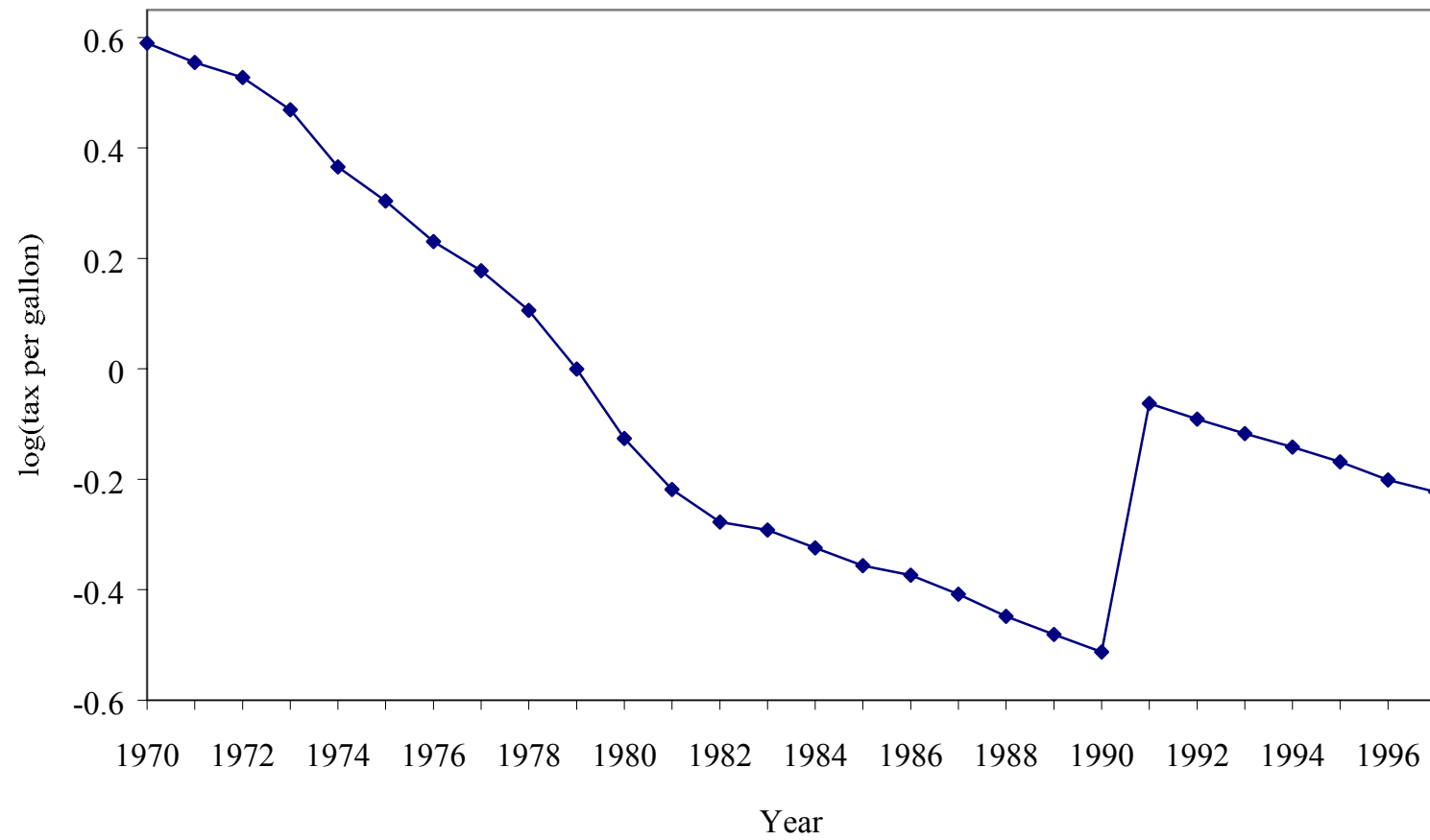


Figure 5: Beer Taxes Over Time: 1970-1997

**Figure 6: Differences in Changes in Beer Taxes
(States with Elections Versus States with no Election)**

